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The institution of these two societies was almost coincident with the opening of the three Universities of Kharkof, Kazan and Vilna by the Emperor Alexander I. Before his reign there had been but one university in the Empire, that of Moscow. Towards the close of his reign, in 1819, still another university, that of St. Petersburg, was opened; and in the same year the Society of German Practitioners in St. Petersburg was formed. This was followed a year later by the formation of a similar society in Warsaw. A gap of over thirty years, corresponding to the reign of the Emperor Nicholas I., succeeded, during which but one medical society was instituted in Russia. This was the Society of Russian Practitioners in St. Petersburg. From the date of the accession of Alexander II. the number of these societies has rapidly increased. There are now no fewer than 120 in the whole of the Empire, the total capital of which amounts to over \$600,000. The majority of Russian medical societies have libraries attached to them; while some have museums, laboratories and even free dispensaries.

UNIVERSITY AND EDUCATIONAL NEWS.

It is announced that M. Eulogius Georgieff, the founder of the Sofia University, who died recently, bequeathed 20,000,000 fr. to the Bulgarian government for public purposes, including 6,000,000 fr. for a technical school to be established at Sofia.

MR. S. B. BROWNELL has presented Barnard College, New York, with a building for a dormitory to cost \$150,000, which will be erected at once.

THE endowment left by Patrick B. O'Brien, of New Orleans, for establishing chairs of chemistry, of physics and of Roman law in the Catholic University of America will be available this autumn. The Rev. Father W. J. Kirby has been appointed professor of sociology.

A COMMITTEE of the Trustees of the University of Missouri has been appointed to investigate the political views of Professor C. H. Hicks, since 1892 professor of political economy in the University. The report of the committee will probably result in the removal of Professor

Hicks, and it is said that Dr. W. G. Brown, professor of chemistry, is likely to share the same fate.

THE Council of the University of Lyons has devoted the sum of 42,000 fr. to complete the construction of the laboratory of biology of Tamaris, near Toulon, and it is expected that a sum will be appropriated to the support of the laboratory.

THE School of Applied Chemistry, founded last year under the auspices of the faculty of science of the University of Paris, has celebrated its first anniversary. M. Darboux presided, and an address was made by M. Friedel, director of the laboratory.

THE University of Berlin offers holiday courses for teachers of science in the higher schools from the 29th of September to the 9th of October of the present year. Lectures will be given by Drs. van't Hoff, Dames and other leading professors, and excursions and visits of scientific interest have been arranged.

DR. KIHLMAN has been appointed associate professor of botany in the University at Helsingfors. Dr. Theodor Petersen and Dr. Josef Epstein, of the Physical Institute of Vienna, have been appointed to professorships. Mr. Stanley Dunkerley, of the department of applied mechanics, Cambridge, has been appointed professor of applied mechanics at the Royal Naval College, Greenwich.

DISCUSSION AND CORRESPONDENCE.

'THE PRESENT EVOLUTION OF MAN.'

IN SCIENCE of July 2d, pp. 33-35, Professor T. D. A. Cockerell published a kind and appreciative review of my book 'The Present Evolution of Man,' for which I am more than grateful. He disagreed with me, however, as to several matters, and as on these precisely I have failed to carry conviction to many critics I am forced to believe either that I am wrong or that my explanations have not as yet been adequate nor sufficiently clear. I would fain hope the latter, and therefore am glad to seize the opportunity Professor Cockerell's review affords of making some reply in the pages of SCIENCE.

Professor Cockerell does not entirely agree with my theory of retrogression. Put very

briefly, that theory is as follows: It is known that the ontogeny recapitulates the phylogeny, though in a rapid, blurred and indistinct way.* In other words, almost every individual recapitulates the traits of all his ancestors, beginning with those of the first (as, for convenience, we may call the unicellular organism) and ending with those of the last. I use the word 'almost' because, though the earlier stages must, of course, be recapitulated, for otherwise the individual would not develop, yet sometimes (indeed often) the very last steps made in the evolution of the race fail to be reproduced by the individual in his development. He then resembles some remote ancestor more than he does his parents, and presents, in fact, an example of what is known as atavism. When the evolution of the race has been rapid, reversion to any given ancestor results, of course, in a much greater and more observable degree of retrogression than when the evolution has been slower, and therefore, while offspring of race horses often exhibit far-reaching reversion towards the ordinary horse, and while the seedlings of various garden plants (apple, peach, rose, etc.), which have been evolved under excessively stringent selection, generally revert to the ancestral type, the offspring of wild plants and animals generally 'breed true.'

An individual may vary from his parent in two ways—towards the ancestry when some of the last steps made in the phylogeny are omitted in his ontogeny, or away from it, the former variation being atavistic, the latter evolutionary; and so far as we are aware the chances of his doing the one or the other are equal. But while every variation towards the ancestry, that is, every failure to repeat in the ontogeny the last steps made in the phylogeny, produce atavistic retrogression, every variation from it need not be an extension of the previous evolution; it may constitute a reversal of it (as will be seen presently) or be in some other different direction, whence it follows that in the absence of selection a species must always undergo retrogression.

Up to this point, or nearly up to it (for as to

*I have given reasons why the recapitulation is blurred and indistinct, but these need not detain us here.

this I am not very clear), my critic and I seem to be in agreement; but beyond this we differ; for while he thinks that such atavism can result in very limited retrogression only, I am of the opinion that in the entire and continued absence of selection it must result in absolutely unlimited retrogression. Before dealing with his objections I ought to say that in my book I quite reject the Lamarckian doctrine of the transmissibility of acquired traits, and since Professor Cockerell is good enough to say that he regards the arguments there used as conclusive, the matter is not in dispute between us. Moreover, he seems to agree with me in attributing evolution to the accumulation of small 'normal' variations, not to the accumulation of great and abnormal variations, for the reason that the latter tend to be swamped, owing to their infrequency. I should also make it clear that by atavism I do not necessarily mean atavism of the whole organism. Such wholesale reversion to the ancestral type must be extremely rare in the case of all species slowly evolved under the ordinary conditions of nature; it occurs only, so far as I am aware, in such species as have evolved under very stringent artificial selection. Under natural conditions an individual may exhibit evolution in some particulars and atavistic retrogression in others, but these latter, owing to the slowness of the antecedent evolution, must generally be minute in amount, and, therefore, when associated with evolutionary changes, unrecognizable in the individual, though recognizable after the lapse of generations in a line of individuals. Thus, while selection may result in evolution in the legs of a bird to which flight has become useless, atavistic reversion in the absence of selection as regards them would result in the retrogression of its wings. But here again, the wings, being complex organs, would not equally retrogress in all their parts, and, therefore, would never very closely approximate to the ancestral type.

Professor Cockerell says: "I cannot see, with Mr. Reid, that there would be unlimited atavism, because when the atavistic changes had proceeded from B to A *the B features would become ancestral* and a new atavism from A to B would appear." (The italics are mine.) It is

this sentence which leads me to suppose that I have not adequately explained myself, for clearly Professor Cockerell does not understand my theory as I understand it. Supposing, as regards any character, A B C D represent a line of individuals, then, if D reverts to B, that is, if D varies from C so that in his ontogeny he repeats the life history of the race only up to the point reached by B, omitting the additional characteristics of C, my contention is that C disappears altogether from the series, which, from the point of view of heredity, becomes A B D and, therefore, if the characters of C ever reappear in E, or any subsequent member of the series, they must do so (as they did in C's case) as a result of evolution (variation away from the ancestry), not as a result of retrogression, of atavism (*i. e.*, variation towards the ancestry).^{*} To test the truth of the theory we must consider a species in which evolution has been very rapid, as in that case atavistic changes can easily be observed with comparative ease; for example, any one of the various garden plants which are propagated by means of slips, it being probable that but a few seminal generations intervene between these widely divergent types and the ancestral wild varieties. The seeds of such a cultivated plant usually give origin to plants, which, in the great majority of instances, resemble much more nearly the wild than the cultivated variety. Now, if Professor Cockerell is right, the continual seminal propagation of such reverted plants should often result in a 'reversion' to the cultivated type; whereas, if I am right, this 'reversion' should never occur, since (for purposes of heredity) the cultivated variety has been eliminated from the ancestral line. Such reappearance would purport, therefore, not reversion, but a fresh evolution, which evolution would be too great to be accomplished in a single generation. So also the race horse, in the absence of selection, would revert to the ordinary horse, after which a race horse should be as rare among the

progeny as it is at present among the progeny of ordinary horses.

It follows as a logical conclusion from the foregoing that cessation of selection, as regards any structure or character, during unlimited time must result in unlimited atavism. First recent ancestors would be approximated to, then ancestors more remote, and lastly that remote ancestral condition when the structure or character did not exist. It would then disappear as utterly as have done, from this cause, the limbs of serpents. In fact, atavism effects the same result as disuse is said to do by Neo-Lamarckians, and, unless we accept the Lamarckian doctrine, I confess I am unable to perceive any other probable reason for retrogression.

Professor Cockerell further writes: "The germ, it must be supposed, contains units representing many phases of existence, while others are new. When one of the latter develop we say the variation is a progressive one; when the former develop we call the result atavism. It is reasonable to suppose that environmental and germinal selection are the factors which determine which of its possible developments the germ shall undergo." Here, again, is proof that I have not made myself clear on this point to Professor Cockerell. He has been kind enough to speak with some approval of my theory, but he has not perceived that it is absolutely incompatible with the theory of germinal selection. Surely in the present state of our knowledge it is too much to affirm that the germ *must* contain *units* 'representing phases of existence.' During the ontogeny many phases of existence are represented in fleeting but orderly succession, but again, surely it is not essential to suppose that the development of each is due to one or more *units*; and, moreover, does not the regular recapitulation of the phylogeny during the ontogeny negative the idea that any 'units' are 'held over'?

I must be brief in my comments on the rest of Professor Cockerell's review. He says that in considering man's present evolution I have neglected to take into account what Mr. B. Kidd has called 'social efficiency.' If he turns to my work (pp. 178-200) he will find that I have not altogether neglected the matter. Briefly, I have attributed differences in social

^{*} For the sake of simplicity I do not here consider such a case as when D repeats the life history up to C and then reverts back to B, but it is dealt with to some extent in my book, and the process is of importance when considering the phenomena connected with 'reversed selection.'

efficiency to differences in acquired mental characters, that is, to differences in education. Mr. Kidd attributes social efficiency, which he derives from the greater or lesser development of the altruistic feeling, to natural selection; but natural selection implies elimination of the unfittest, and he has failed to record a single death as due to the absence of this feeling in him who perished and the presence of it in him who survived. If it be maintained, as is sometimes done, that 'social efficiency' has been evolved, not through selection of individuals, but through selection of communities, then I can only say that such a contention appears to me to involve a complete misconception. Evolution can result only from the selection of individuals, never from the selection of communities; except, indeed, in the case of such communities (*e. g.*, ants, bees, termites) as are the progeny of a single pair of individuals, when the parent individual is selected in the person of the progeny. Supposing a superior community (of men, for instance), without individual selection within itself, causes the elimination of an inferior community, then, as a consequence, the former spreads, but does undergo evolution. Its superiority (if inherent, and not acquired) arises from an antecedent selection of individuals within itself. Professor Cockerell says that sanitary arrangements 'do select the citizens of one town, state or country as against others, or those of the rural districts as against the towns.' But surely sanitary arrangements do not come under the head of human evolution, but under what has been called 'Evolution in the Environment.' The knowledge of it is acquired. If New York now values sanitation more than it did fifty years ago, or more than Baltimore does at the present time, is this not due, beyond doubt, to a difference in education (*i. e.*, acquired mental characters), not to an inborn intellectual difference?

I now come to the last of Professor Cockerell's objections and I wish exceedingly I had more space to deal with it. I have shown that certain powerful narcotics (*e. g.*, alcohol and opium) are great causes of elimination; that races (*e. g.*, Greeks, Italians, South Frenchmen, Spaniards, Portuguese) which have long possessed a cheap and abundant supply of alcohol,

for instance, are the least prone to excessive indulgence of all races on earth; that other races, (*e. g.*, Anglo-Saxons, Scandinavians, Russians, etc.), which have had a less extended experience are more prone to intemperance; that yet other races (*e. g.*, savages of all kinds, whether inhabiting the frigid, the temperate or the torrid zones), who have had little or no experience of alcohol, crave for that narcotic so intensely that, in the presence of an abundant supply and the absence of prohibitory laws, they perish of excessive indulgence; and have argued, firstly, that the craving for narcotic indulgence was inborn in man as a by product of mental evolution;* secondly, that the Italian, for instance, is more temperate than the American Indian, as a result of natural selection, *i. e.*, alcoholic selection; and thirdly, that to render a race more temperate we must eliminate, not drink, but the excessive drinker, for compulsory temperance must lead to the survival of the unfit and consequent retrogression of the race to the ancestral type, when the craving was stronger than it now is in a race which has undergone alcoholic selection.

Had I proved my facts and used this line of argument as regards any physical structure I think all the world would have agreed with me; but, because I dealt with the burning question of intemperance, I have met with numerous objectors ranging from a clerical gentleman, who found distinct points of resemblance between Satan and me, and argued that, *therefore*, my theory 'must be a lie,' through Professor Ray Lankester, who, while admitting the truth of the theory, apparently thought that a tendency to get excessively drunk might be necessarily correlated to extremely valuable quali-

* Just as the paresis which accompanies excessive fear is inborn, *e. g.*, in frog when attacked by snake, as a by product, a correlated variation, of the life saving faculty of fear. Of course, I do not mean that an American Indian, for instance, who has never tasted alcohol, craves knowingly for it; I merely mean that he has an inborn love for such feelings as are induced by deep indulgence, just as he has an inborn love for such feelings as may be induced by eating a delicious food. Till he tastes a peach, for instance, of course he does not knowingly crave for it, yet nevertheless the love for the peach is born and in like manner is that for alcohol.

ties, up to Mr. Alfred Russell Wallace and to Professor Cockerell, who thinks "that the human race has no natural craving for alcohol at all, but it has a craving for excitement and other states of mind which may be induced artificially, and that when the natural exercise of highly valuable faculties is denied, as is so often the case in our present civilization, artificial means, often highly injurious, will be resorted to." But it is the absolute savage who is most prone of all races on earth to excessive indulgence, and surely, living, as he does, in a 'state of nature,' the natural exercise of 'highly valuable faculties' is not denied to him. Moreover, if Professor Cockerell is right, what is the difference between the civilization of the South and North of Europe, which permits in the former case the natural exercise of valuable faculties, but forbids them in the latter, for the north Europeans are much more drunken than the south Europeans.

Let the reader think awhile. Why does he not get drunk? Is it because he constantly resists the craving, or because the craving does not exist in him? I think he will say, 'the latter.' But has he no acquaintance, reared and living under much the same conditions, who drinks, to excess, though all his interests call him to abstain? I think he is sure to have such an acquaintance. Now, in this respect nations like the Italians or the Spaniards are mainly composed of individuals like my reader, while nations like the American Indians or the native Australians are mainly composed of individuals like his unfortunate acquaintance.

Here is a significant fact: old records seem to prove that the classic races were anciently much more intemperate than at the present time. For instance, the temperance question was formerly a burning one in Greece, where unhappy Helots were made to furnish 'awful examples' to the aristocratic youth. Here is another: the deadly narcotic opium has been in use for some hundreds of years in India, and never or very rarely does a native of that country take it to excess; it has been in use for about two hundred years in China, and most of the Chinese are temperate, though some take it to excess; it has been recently introduced into Burmah, and, practically speaking, all

Burmans take it to such excess that they perish of it, and, therefore, in their own country the English have forbidden the use of opium to Burmans alone, while permitting it to all other peoples, just as in Canada alcohol is forbidden to the aborigines alone. Here is a third: tobacco causes little or no elimination, and, therefore, the craving for it is as strong in races that have longest used it as among races to which its use is comparatively strange.

G. ARCHDALL REID.

LOUTHSEA, ENGLAND.

AMPHIBIA OR BATRACHIA.

PROFESSOR BURT G. WILDER has made some remarks in the last number of SCIENCE (August 20, 1897) about the French word *Batraciens*. He says: "Dr. Baur shows that the French word *Batraciens* was applied to the frogs, toads and salamanders by Brogniart in 1799, and that the Latin forms *Batrachii* and *Batrachia* were not introduced until 1804 and 1807, by Latreille and Gravenhost. But does not Dr. Baur lay undue stress upon the distinction between the French and the Latin form? *Batraciens* is not (like *crapaud*, etc.) a vernacular word; it is the French form, or, galloparonym (!), of the Latin *Batrachia*, and the employment of the former would seem to constructively sanction the use of the latter." Professor Wilder 'as a teacher of zoology, but without claim to expert authority upon taxonomic points,' seems to be absolutely ignorant of the fundamental rule in nomenclature (published in all Codes of Nomenclature), that all vernacular names, of genera, families, orders, classes, even if formed from a classical root, are never accepted. Such vernacular names have especially been used in France by Cuvier, Lesson, de Blainville and notably other French writers of the early part of the present century. Such names have in many cases been later adopted into the science under a proper classical form, and should take date only from this later introduction. I should like to recommend to Professor Wilder the study of 'The Code of Nomenclature adopted by the American Ornithologists' Union, 1892.' This code is followed by all American naturalists. The case of *hippocampus* referred to by Professor